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An Experimental Inquiry into the Effect of Yardstick Competition on Corruption

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ABSTRACT

This study reports theory-testing laboratory experiments on the effect of yardstick competition on corruption. The results reveal that on the incumbent's side, yardstick competition acts as a corruption-taming mechanism only if the incumbent politician is female. On the voter's side, voters focus on the difference between the tax rate in their own jurisdiction versus that in another jurisdiction. If the voters' tax rate is deemed unfair compared to that in the other jurisdiction, voters are less likely to re-elect. These findings support the claim by Besley and Case (1995) that incumbent behavior and tax setting are tied together through the nexus of yardstick competition, suggesting that our laboratory experiments have some external validity.

Key words: corruption, yardstick competition, political agency, asymmetric and private information, experiments

1. INTRODUCTION

According to the Organization for Economic Cooperation and Development, the Nigerian President recently estimated the total cost of corruption in Nigeria at a quarter of Africa's total income. 1 Nigeria is not alone. As suggested by the indices of Transparency International, many countries are presently coping with the problem of corruption.2 Similar to other forms of economic behavior, corrupt behavior can be seen as the result of an interaction between an agent's environment and the institution in which that agent makes decisions.3 Thus, it is interesting to study how institutions affect corrupt behavior.

The nature and causes of corruption have been studied in many contexts.[4] Recently, economists have become interested in a group of nearly self-correcting mechanisms for political corruption, which fall under the category of fiscal decentralization (e.g. Bardhan and Mookherjee, 2005). This study addresses the efficacy of one such mechanism, namely whether yardstick competition has an effect on incumbent and voter behavior, and if so, in what direction.

Besley and Case (1995) were among the first to define yardstick competition in public economics. Yardstick competition between incumbent politicians arises when voters in one jurisdiction use the performance of incumbents in other jurisdictions as a benchmark for their own incumbent's performance. This has effects on both sides of the equation. In the presence of yardstick competition, voters have larger information sets, and thus tend to make more informed voting decisions. On the incumbent's side, the knowledge of the existence of yardstick competition—i.e. the fact that incumbents know they are being compared to a benchmark—gives rise to performance-based competition.

Yardstick competition can act as a corruption-taming mechanism by imposing fiscal restraints on incumbent behavior. Incumbents typically desire re-election, and lower taxes imply a higher likelihood of re-election; thus, incumbent behavior and tax setting are tied together. Furthermore, a malevolent (i.e. corrupt) incumbent is more likely to exercise fiscal restraint in the presence of yardstick competition if a sufficiently large proportion of good incumbents exist in other jurisdictions. This is how yardstick competition ties together incumbent behavior (i.e. vote seeking) and tax setting.

The theoretical link between yardstick competition and corruption has been addressed in several studies, including Besley and Smart (2007) and Belleflamme and Hindriks (2005). Both of these models posit that yardstick competition gives rise to a "discipline effect" related to moral hazard, and a "selection" effect related to adverse selection. However, while these effects are reinforcing in the Belleflamme and Hindriks (2005) model, they are competing in the Besley and Smart (2007) model. As a consequence, the latter model finds that the effect of yardstick competition on voter welfare is ambiguous, whereas the first model finds that yardstick competition always exercises fiscal restraint and thereby increases voter payoffs.

Empirically, the link between yardstick competition and corruption has been addressed using field data (e.g. Besley and Case, 1995; Dincer et al. 2006; Johnson 2006). While the first two references find strong evidence of yardstick competition on incumbent behavior and tax setting, the latter study does

[1] The World Bank (1997) defines corruption as: "The abuse of public office for private gain." Jain (2001) proposes a similar definition.

[2] Corruption is termed problematic because there is a generally widespread consensus among (social) scientists across many disciplines that corruption is costly to society, since it hinders economic growth and thus promotes poverty and income inequality. See Mauro (1995).

[3] There are many ways to define the terms "environment" and "institution." This paper adopts the definitions proposed by Smith (1989). The environment consists of the collection of all agents' characteristics, i.e. their tastes and technology, which in traditional economics are represented by utility or preference functions, resource endowments and production or cost functions. The institution defines the language (messages or actions) of communication, and also specifies the order in which economic agents move (or that there is no form and moves are free-form), and the rules under which messages become contracts, and thus allocations.

[4] Some general survey pieces include Martinez-Vazquez et al. (2007), Aidt (2003), Tanzi (1998) and Bardhan (1997).

not. As reviewed by Abbink (2005) and Dusek et al. (2005), no experimental study has directly addressed this question to date.[5]

Therefore, the present study contributes to the literature in the following ways. First, it provides experimental data on the effect of yardstick competition on incumbent and voter behavior and welfare. In other words, we study the efficacy of yardstick competition as a mechanism. See for example Shleifer (1985) and Holmstrom (1982) for a theoretical discussion of yardstick competition as a mechanism. This is relevant both in economics (public economics, industrial organization, mechanism design, etc.) and in other fields such as political science.

Secondly, we address the internal versus external validity of yardstick competition as a mechanism, by comparing the results of this study with the seminal study by Besley and Case (1995). This gets to issues of generalizability. Finally, the study contributes to the experimental literature on sender-receiver signaling game protocols in which agents try to reveal or conceal their types by means of the signals they send. This includes studies on (1) the plausibility of sequential equilibria in signaling games (e.g. Brandts and Holt, 2005); (2) the lemon's phenomenon (e.g. Miller and Plott, 1985); and (3) signaling in miscellaneous contexts including voluntary contributions (e.g. Potters et al., 2007), reputation building (e.g. Grosskopf and Sarin, 2006) and board composition (e.g. Gillette et al., 2003).

To address the effect of yardstick competition on incumbent and voter behavior, we conduct theory-testing laboratory experiments using baseline and yardstick treatments (conducted between subjects), both of which are informed by Besley and Smart (2007). The baseline treatment is based on a two-period sequential signaling protocol in which the incumbent chooses a pair of first-period monetary payoffs for both the incumbent and the voter (see below).[6] These monetary payoffs are derived from explicit parametric specifications of the game-theoretic payoff functions, which are based on four key items: (1) the unit cost of the public good (θ); (2) the level of public good provision (G); (3) the tax rate (t); and (4) the marginal cost of public funds (μ).

The baseline treatment proceeds as follows. The incumbent first chooses between alternative payoff pairs. If the incumbent is benevolent (i.e. non-corrupt), he or she chooses the alternative associated with equal first-period payoffs. By doing so, he or she maximizes voter payoffs. In contrast, if the incumbent is malevolent (i.e. corrupt), he or she chooses an alternative that is associated with reduced (and unequal) voter payoffs. The voter observes his or her first-period payoff and the range of possible first-period payoffs for the incumbent. The voter does not know the incumbent's first-period payoff with certainty due to imperfect information. This is achieved by having a move of nature (in the sense of Harsanyi, 1967) at the beginning of the game protocol. Having received this information, the voter decides whether or not to re-elect the incumbent. If re-elected, the incumbent advances to the second period and chooses another pair of second-period payoffs, whereupon the game comes to an end. If voted out, the incumbent gets a second-period payoff of zero and the voter's second-period payoff is determined by a lottery, whereupon the game ends.

The yardstick treatment is based on the identical game protocol with one minor modification. The voter observes an additional piece of information (i.e. yardstick information): namely, the first-period payoff of another voter drawn at random from the distribution of first-period payoffs of all voters in another jurisdiction. This is the only difference between the baseline and yardstick treatments, and it indicates the presence of yardstick information, i.e. a benchmark payoff (or tax rate).

[5] Potters et al. (2004) studied how collusion affects yardstick competition. The aim of their study differed from that of our study in two main aspects. First, collusion is different from the type of corruption studied here, since there is no "third party" with which the incumbent can collude. Second, even if one argues that collusion is similar to corruption, their question remains different, since they ask whether collusion affects yardstick competition as opposed to the other way around.

[6] Weibull (2004) draws the distinction between a "game" and a "game protocol." A game is that which is theoretically defined based on true game-theoretic payoffs (traditionally thought of as Bernoulli utility functions). A game protocol, in contrast, is generally tested in the laboratory or some other applied environment, and is usually based on monetary payoffs. When describing the theoretical model, we use the term "game" herein. When describing the experiment, we use the term "game protocol." Unless explicitly stated otherwise, the term "payoffs" refers to monetary payoffs.

The main findings are the following. The study finds strong evidence of yardstick competition. On the incumbent side, yardstick competition acts as a corruption-taming mechanism if the incumbent politician is female. Also, yardstick competition makes incumbents more aware of their choices, since beliefs matter more in the presence of yardstick competition. On the voter side, voters focus on the difference between the tax rate in their own jurisdiction and that in the other jurisdiction when making their re-election decisions. In particular, as the discrepancy between the taxes (i.e. first-period payoffs) increases, voters are less likely to re-elect.

The experiments also find some partial effects that determine corruptibility. On the incumbent side, the unit cost of the public good matters. If the unit cost is high, incumbents are more likely to charge lower taxes. Also, gender is important when interacted with the unit cost. Female incumbents are more likely to divert rents and charge maximum taxes when the unit cost is high. Finally, incumbents' beliefs towards re-elections are important; in particular, they weigh the likelihood of re-election heavily when choosing tax rates. On the voter side, beliefs are captured by the tax rate, which signals the incumbent's nature. In particular, voters re-elect more frequently when taxes are lower. Also, female voters behave more consistently with the pooling equilibrium in the Besley and Smart (2007) model.

Overall, the findings support the seminal study by Besley and Case (1995) in their claim that incumbent behavior (i.e. vote-seeking) and tax setting are tied together through the nexus of yardstick competition, suggesting that the findings of these laboratory experiments have some generalizability.

The remainder of the paper is organized as follows. The next section discusses the design of the experiments, followed by an exploration of the main results. Finally, we conclude. The subject instructions are given in the appendix.

2. EXPERIMENTAL DESIGN

The Game

The theory is based on a game-theoretic model of elections that is cast in a principal-agent framework. There are two "active" players [a principal (i.e. the voter) and a first agent (i.e. the first-period incumbent politician)], and one "passive" player [a second agent (i.e. the second-period challenger)]. An agent's type (i) can be good (g) or bad (b). The good type always behaves non-strategically and sets rents (i.e. corruption) equal to zero. The bad type always diverts some rent (i.e. behaves corruptly). Each first agent knows his or her own type; however, the principal and the first agent do not know the second agent's type.

The model comprises two periods, with an election at the end of the first period. The second agent is "passive" in the sense that he or she only plays a role during the election. The election guarantees that decisions made in the first period have consequences for the second period. This describes a dynamic game of incomplete information, since (1) actions in the first period affect game-theoretic payoffs in the second-period, and (2) at least one player is uncertain about another player's game-theoretic payoffs.

The game can be transformed into a dynamic game of imperfect information by introducing moves of nature that determine types (Harsanyi, 1967). Information revelation proceeds as follows.

In the first period:

1. Nature draws the types (i in {g=good,b=bad}) of the incumbent and the challenger from i.i.d. distributions such that $P(i=g) = \pi$. This information is common knowledge.
2. Nature reveals the politician's type only to the respective politician.
3. Nature draws the unit cost of the public good (θ), which may be either high (H) or low (L), from i.i.d. distributions such that $P(\theta=H)=q$ and $P(\theta=L)=1-q$, where $H>L$. This information is common knowledge.
4. Nature reveals the unit cost of the public good (θ) only to the incumbent politician. The incumbent's type–which is private information–is determined by two components (i,θ): whether he or she is by nature good or bad, and whether the unit cost of public good provision is by nature high or low.
5. Based on the incumbent's knowledge of his or her type (i,θ) and the associated game-theoretic payoffs, the incumbent chooses the level of public good provision (G) and the amount of rent diversion or corruption (r), which together determine the total tax collection (t) by means of the equation, $t=\theta G+r$. The voter pays t. Under the assumption that there is some maximum level of tax collections (T) in any given period, the incumbent politician's action set is $A_I=\{(G,r) \mid \theta G+r=t, \text{ t in } [0,T], G,r\geq 0\}$.
6. The voter initially observes two signals, the level of public good provision (G) and the total tax collection (t), but cannot infer the level of corruption because the unit cost of the public good (θ) is private information.
7. At the end of the first period, after the voter has observed the public good-tax signal (G,t), there is an election. The voter decides whether or not to re-elect the incumbent by comparing the signals received from the "active" agent (the first-period incumbent politician) with the prior belief that the "passive" agent (the second-period incumbent politician) is good.[7] So, the voter's action set is $A_V=\{\text{re-elect, not re-elect} \mid \Gamma\}$, where Γ is some information set consisting of: (1) the level of public good provision (G); (2) the tax collection (t); (3) the commonly known probabilities that the unit cost of the public good is high or low (i.e. q and

[7] The model assumes that one voter determines whether or not the incumbent is re-elected to office. While this may seem contrary to standard majority-voting models, we can interpret this model as a median-voter or representative-voter type model in which the voter is decisive or representative of the majority vote.

1-q); (4) the commonly held prior beliefs that the incumbent or challenger is good or bad (i.e. π and $1-\pi$); and (5) the posterior belief that the incumbent is good (p). The voter creates this belief through Bayesian updating.

In the second period:

8. If the voter chooses to re-elect, the incumbent politician remains in office. If the voter does not re-elect, the challenger takes over the office. Recall that the challenger's type was already determined by nature in step (1) and revealed accordingly.
9. Nature draws the unit cost of the public good (θ) as described in step (3) and reveals it accordingly.
10. The second-period incumbent chooses the level of public good provision (G) and the level of corruption (r), which together determines the tax collection (t).
11. The voter observes the level of public good provision and taxes, pays his or her taxes, and the game comes to an end. There is no election at the end of the second period.
12. The players' game-theoretic payoffs are determined by the following functions. Voter welfare is given by $W(G,t)=G-\mu C(t)$, where G and t are as previously defined, μ is an exogenous parameter indexing the marginal cost of public funds, and C is a strictly convex and increasing function with $C(0)=0$.[8] Politician welfare is type-dependent. A good politician only cares about voter welfare, and thus has the same game-theoretic payoff function as the voter. A bad politician does not have the voter's best interests at heart and always diverts rents (i.e. behaves corruptly). Given a discount factor of $0<\beta<1$ and a re-election probability of ψ, a bad politician's game-theoretic payoff function is $R=r_2+\beta\psi r_2$, where R is the discounted expected rent diversion (i.e. corruption) and r_j is the level of corruption (rent diverted) in period j for j=1,2.

This type of game is most easily solved using a backward induction argument, giving rise to perfect Bayesian equilibria (PBE). Since the good type behaves non-strategically by always setting rents (i.e. corruption) equal to zero, thereby maximizing voter welfare, it is only necessary to characterize equilibrium behavior for the bad incumbent. Furthermore, since the bad incumbent always diverts some rent, a high unit cost of public good provision automatically implies that he or she is detected ex post (i.e. at the time of re-election). Consequently, equilibrium behavior says that the voter will never re-elect the bad incumbent when the unit cost of public good provision is high, and the more complicated equilibrium characterizations are associated with the behavior of a bad incumbent facing a low unit cost of public good provision—i.e. type (b,L).[9]

Three types of equilibria are characterized: (1) a pooling equilibrium, (2) a separating equilibrium, and (3) a hybrid equilibrium. The pooling equilibrium corresponds to the case in which it is optimal for type (b,L) to restrain rent diversion (i.e. corruption) and mimic the behavior of a good incumbent facing a high unit cost—i.e. type (g,H). In this case, they both send the same public good-tax signal, meaning that the voter cannot tell the difference between type (b,L) and type (g,H), and re-elects the incumbent.

The separating equilibrium corresponds to the case in which the expected game-theoretic payoffs in the second period are sufficiently low that it is optimal for type (b,L) to divert maximum rents in the first period. In this case, type (b,L) separates from type (g,H), since the voter observes a different public good-tax signal depending which type is in office. In particular, if type (b,L) is in office, the voter gets charged maximum taxes (T), type (b,L) is detected with certainty ex post, and the type (b,L) incumbent is voted out.

[8] Browning (1976) defines the marginal cost of public funds as the direct tax burden plus the marginal welfare cost produced in acquiring the tax revenue.

[9] Besley and Smart (2007) characterize the PBEs associated with type (b,L). We describe these equilibria intuitively below. For a rigorous derivation, the reader is referred to their paper.

Finally, the hybrid equilibrium corresponds to a mixture of the two types of equilibria described above. In this case, type (b,L) adopts a strictly mixed strategy between fiscal restraint and maximal rent diversion. There is some probability that the voter will observe one of two signals from type (b,L). The voter re-elects with strictly positive probability, not equal to one.

The above equilibrium strategies describe behavior in the baseline game without yardstick competition. This baseline game informs the design of the baseline treatment discussed in the next section. In addition, Besley and Smart (2007) describe a theoretical environment with yardstick competition. Their environment informs the design of the yardstick game protocol. In the game with yardstick competition, there are two jurisdictions—home and foreign—instead of the single jurisdiction used in the baseline game. The jurisdictions are symmetric in the sense that the joint probability mass function of cost shocks $P(\theta,\theta')$ is symmetric. Furthermore, it is assumed that the cost shocks in the two jurisdictions are positively correlated by letting $P(H,H)=P(L,L)=\rho/2$ and $P(H,L)=P(L,H)=(1-\rho)/2$, for $\rho>½$.

Besley and Smart (2007) derive propositions that describe equilibrium behavior in the presence of yardstick competition, with the yardstick competition case differing from the baseline case as follows. When the domestic incumbent of type (b,L) exercises fiscal restraint (as is the case in the pooling equilibrium) he or she is less likely to be re-elected if the foreign incumbent is type (g,L). Thus, the bad incumbent is less likely to fool the voter by exercising fiscal restraint, if the foreign incumbent is of the good type and chooses low taxes. Furthermore, type (g,H) is more likely to be retained in office when the foreign incumbent chooses maximal rent diversion (i.e. behaves most corruptly). Finally, voters may be worse off under yardstick competition if there is a sufficiently large proportion of bad incumbents within their jurisdiction. In the latter case, bad incumbents are likely to be replaced by equally bad incumbents, and yardstick competition functions neither as a corruption-taming mechanism nor as a meaningful information mechanism.

Treatments: The Game Protocol

The experimental design is based on two treatments, a baseline treatment (Treatment B) and a yardstick treatment (Treatment Y). These treatments are based on baseline and yardstick game protocols that constitute experimental implementations of the baseline and yardstick games described above. The baseline game protocol is described in detail below. The yardstick game protocol (and thus, the yardstick treatment) differs subtly from the baseline protocol (and treatment) by what is observed by the voter. In the yardstick game protocol, the voter observes additional information, as is the case in the yardstick game. In particular, the voter observes another voter's first-period payoff (i.e. the experimental "equivalent" of the foreign public good-tax signal), which was drawn at random from the distribution of first-period payoffs in the baseline game protocol. The experiments are between-subjects in the sense that any given pair of subjects (incumbent-voter) participates in one and only one of these two game protocols (and thus, treatments). In both treatments, incumbents are faced with the exact same set of choices.

Several issues arise when transitioning from theory to experiments. First, since experimental subjects are known to exhibit learning throughout the course of the experiment, it is typical to introduce repetition. Therefore, we design the baseline treatment such that it consists of ten repetitions of the baseline game protocol, which in turn is an experimental implementation of the one-shot baseline game. In order to minimize repeated-play effects, subjects are informed and guaranteed that they will not interact with the same player for more than one repetition.

Second, the baseline game contains two moves by nature: one that determines whether politicians are good or bad (i in {b,g}), and one that determines the unit cost of the public good (θ). Together, these two pieces of private information determine a politician's type (i,θ). When implementing the baseline game protocol, the question arises whether or not we want to induce politicians' types as part of the experiment. The answer to this question depends on what we hope to learn from conducting the experiment. Since part of our main question is the extent to which yardstick competition affects incumbent behavior, it does not make sense to induce benevolence. Therefore, we allow the first component of incumbent subjects' types to be homegrown—i.e. as it is when they enter the experiment. However, we induce the level of the unit cost of the public good (see below for further discussion).

Third, incumbent and voter subjects face alternatives that are "on-the-equilibrium-path" according to Besley and Smart's (2007) characterization of PBEs. In other words, they are not presented with payoffs that do not correspond to the pooling, separating and hybrid equilibria described previously. Finally, in order to simplify the game protocol and since the challenger is a "passive" player, the challenger is implemented as a lottery instead of an actual subject (described further below). The following steps describe one repetition of the baseline game protocol.

In the first period of the repetition: The experimenter flips a coin, which determines the unit cost of the public good (θ). If the coin lands heads-up (i.e. $\theta=1$), the incumbent subject chooses from three alternatives. If the coin lands tails-up (i.e. $\theta=2$), he or she chooses from two alternatives. These alternatives are described in Table 1. Each alternative is a payoff pair that has been calculated based on explicit functional forms game-theoretic payoff functions for both the incumbent and the voter (see below). These alternatives represent actions that the incumbent would make if moving on the equilibrium path, and are therefore analogous to the equilibrium public good-tax signals in the baseline game.

The voter subject observes his or her payoff and the range of possible payoffs for the incumbent, the latter of which are associated with the incumbent subject's choice. The voter subject decides to "accept" (i.e. re-elect) or "reject" (i.e. vote out) the incumbent.

In the second period of the repetition: If the incumbent subject is voted out, he or she gets a payoff of zero, and the voter subject's payoff is determined by a lottery (l) in which he or she has a 25% chance of receiving 1.50, a 25% chance of receiving 0.75, and a 50% chance of receiving 0.16.[10] This represents the challenger's move, which is conditional on the nature-determined type and the lame duck effect.

If the incumbent subject is re-elected, he or she remains in office. The second-period alternatives that he or she chooses from are affected by another draw of the unit cost of the public good. The experimenter flips a coin. Regardless of the level of the unit cost, the incumbent subject faces a choice between two alternatives. However, those alternatives are distinct based on the result of the coin toss as shown in Table 1. In this case, the payoffs for both players are determined by the incumbent's choice.

The voter subject does not get to make a decision at the end of the second period.

These two periods together constitute one repetition. This entire process is repeated ten times. The bad incumbent's per-period payoff function is characterized by $r=t-\theta G$, where all variables are as defined previously. The voter's per-period payoff function (and thus, also that of the good incumbent) is characterized by $W(G, t)=G-\mu t$, where all variables are as defined previously.[11] Finally, the following parameterizations apply to the experimental treatments: (1) since a coin flip determines the level of the public good, $q=½$; (2) the high level of the unit cost is $\theta= 2$ and the low level is $\theta= 1$; (3) the marginal cost of public funds is one quarter (i.e. $\mu = ¼$); and (4) there is a maximum level of tax collection of $T = 4$, associated with a minimum level of public good provision of $G_{min} = 1$.[12]

[10] These amounts represent experimental dollars. In the experiment, one experimental dollar was the equivalent of a quarter of one US dollar.

[11] Besley and Smart (2007) assume that the tax-cost function is strictly convex. Assuming a linear tax-cost function has no particular complications for their results. In particular, the good-type's optimization problem becomes $\max_G [G - \mu\theta G]$, which reduces to $\max_G [G(1 - \mu\theta)]$. If the parameter $(1 - \mu\theta) > 0$, then $G_\theta > 0$. If not, then $G = 0$. In order to have an interior solution, we need μ and θ to satisfy the relationship: $\theta < 1/\mu$. This condition is satisfied in all parameterizations. A linear tax-cost function also leaves the relationships between G and θ and G and μ unchanged. For example, suppose the condition $\theta < 1/\mu$ holds. Now, consider a slight increase in either θ or μ such that θ becomes strictly greater than $1/\mu$. Then, G goes from strictly positive to zero in a discrete manner. Thus, the negative relationships are maintained.

[12] This requirement is not discussed by Besley and Smart (2007). It is introduced here to avoid negative voter payoffs. Based on the formulation of the voter's welfare function, if the minimum level of the public good is zero (i.e. $G_{min} = 0$), the voter automatically has negative payoffs when the incumbent diverts maximum rents. Therefore, we introduce this additional restriction in the game protocol. If the minimum level of the public good is equal to δ (i.e. $G_{min} = \delta$), then existence of the PBEs derived by Besley and Smart (2007) is unaffected as long as δ satisfies the following condition: $\frac{(1-\beta)T-r_1}{H(1-\beta)} \leq \delta \leq \frac{(1-\beta)T-r_1}{L(1-\beta)}$.

The payoff pairs for both periods (see Table 1) are derived using the explicit payoff functions and parameter values discussed above. Consider the first period. If an incumbent chooses alternative 1, we can infer that he or she is behaving like the good type, since this alternative is based on a zero-rent behavioral assumption and leads to equal payoffs. On the other hand, if an incumbent chooses alternative 2 when the unit cost of the public good is low, we can infer that he or she is playing according to the pooling equilibrium, since this alternative is associated with fiscal restraint. Note that when the voter observes a first-period payoff of 0.75, it is uncertain whether this payoff came from a good type that faced a high cost or a bad type that faced a low cost. Finally, if an incumbent chooses any other alternative, he or she is playing according to the separating equilibrium, since maximum rent diversion pushes the voter to the lowest possible payoff.

Now, consider the second period. Since any bad incumbent is a lame duck in the second period, equilibrium behavior would dictate that the bad type will divert maximum rents in the second period because there is no election. Therefore, the bad type always separates in the second period. On the other hand, since the good type always behaves non-strategically, he or she will always equalize the second-period payoffs even though there is no election.

Table 1. First- and second-period alternatives faced by incumbent in Treatment B

First Period			
If Unit Cost is Low (θ = 1)		**If Unit Cost is High (θ = 2)**	
Alternative	**Payoff Pair**	**Alternative**	**Payoff Pair**
A1	(1.50, 1.50)	A1	(0.75, 0.75)
A2	(1.50, 0.75)		not applicable*
A3	(2.35, 0.16)	A2	(1.35, 0.16)
Second Period			
A4	(1.50,1.50)	A3	(0.75,0.75)
A5	(2.35,0.16)	A4	(1.35,0.16)

*Notes: There is no comparable alternative due to the "on-the-equilibrium-path" assumption.

Implementation

The experiments took place in the experimental laboratory at the Experimental Economics Center (ExCEN) at Georgia State University. Subjects were recruited using ExCEN's online recruiter system, which contains the names of students taking courses in many different areas including but not limited to accounting, actuarial science, biology, business administration, chemistry, economics, finance, geology, geography, mathematics, nursing, political science and sociology.

The experiments were programmed and conducted using the z-Tree software package (Fischbacher, 2007). In each experimental session, half of the subjects were incumbents and half were voters. Each experimental session had at least 20 subjects in order to guarantee that a given politician and voter were never paired for more than one repetition. In other words, a given politician did not interact with a given voter for more than one repetition and vice versa. As such, re-matching during the experiment was predetermined according to the "two-ships-passing-in-the-night" design. This means that once you have interacted with a subject, you will never interact with that person again. Subjects were informed accordingly.

Each experimental session consisted of instructions, a five-minute trial, a quiz, a summary of the treatment, the treatment and a post-questionnaire. The experiments lasted an average of 90 minutes. Average payoffs in the baseline treatments were \$15.22 for the incumbent and \$13.59 for the voter. In the yardstick treatments, these were \$15.25 and \$13.08, respectively. Subjects were paid \$5.00 for showing up, \$3.00 for completing the trial and quiz and \$2.00 for completing the post-trial questionnaire.

The procedures during the experiment were as follows. Subjects were assigned a number at sign-in. These numbers were used to randomly enter subjects into the experimental laboratory. Random entry also determined random assignment to a fixed role and different pairs during Treatment B or Treatment Y. After entering the experimental laboratory, subjects were handed neutrally-framed written instructions (see appendix) that were also read aloud by the experimenter. The subjects were then put through a five-minute trial that gave them an opportunity to interact with the software and practice making decisions that did not affect their payoffs. Thereafter, subjects were put through the quiz. Unlike the trial, the quiz was not timed. Subjects were informed that they would earn \$3.00 for completing the trial and quiz regardless of how many questions they answered correctly. They were asked to pay attention to the screens observed in the trial and to the questions asked in the quiz. During the quiz, the software informed subjects whether or not they answered a particular question correctly. In both cases, the quiz gave an overview of the correct answer and referred subjects to the instructions. After the quiz was completed, subjects were asked whether they had any questions that they wanted clarified in private. If so, these questions were clarified as requested.

The experimenter then summarized the treatment, elaborated on the main issues, and addressed any particular issues that seemed to be problematic based on the quiz responses. The problematic issues were generally uniform across sessions. Once this process was completed, subjects were ready to start the main treatment. At this point, each subject was informed whether he or she would be Player X (a politician) or Player Z (a voter).

Subjects were told to note that the room was divided into two “sections” by means of a column of computer stations running from front to back. There were no subjects sitting at those computer stations. This column divided the room into incumbents and voters; everyone on one side of the room was randomly assigned to be an incumbent and everyone on the other side was randomly assigned to be a voter. Within each section, subjects were separated by means of dividers, guaranteeing that they could make decisions in private. Furthermore, the voter side had higher dividers such that it was impossible for a voter to observe the result of the coin toss. This also reinforced the nature of the information asymmetry in the game protocol.

Subjects were told not to communicate with each other during the experiment, and they were reminded that they would keep the same role throughout the entire treatment. At the beginning of each repetition and period within a repetition, the experimenter flipped a coin. The coin toss took place in front of the first two politicians (i.e. Player Xs), who observed and verified the coin toss and its result. Upon verification, the experimenter input the result into the computer and the respective period was conducted. Upon conclusion of the two periods, the process was repeated until all ten repetitions were concluded. After these repetitions, subjects completed the post-trial questionnaire.

3. RESULTS

Aggregation and Demographics

A total of four experimental sessions were conducted: two B sessions and two Y sessions. The main choice (endogenous) variables of interest are first-period choices by incumbents (this variable is called Choice) and re-election (acceptance) decisions by voters (this variable is called Accept). Full distribution tests (Kolmogorov-Smirnov, KS and Epps-Singleton, ES) suggest that there are no statistically significant differences between the sessions for these main variables of interest.13 Aggregation across experimental sessions yields the following number of subjects for the respective treatments: 30 politicians and 30 voters for Treatment B, and 28 politicians and 28 voters for Treatment Y.

The following descriptive statistics describe the average subject profile. Female subjects constitute 56.03% of the sample. The average age is 22 years with a standard deviation of 4 years. About 13% of subjects are economics majors, 12% are biology majors, 8% are accounting majors and 4% are political science majors. The remaining subjects comprise miscellaneous majors including film and video, sociology, gerontology, chemistry and English. Finally, the maximum self-reported annual income range is $30,001 to $60,000 with a mode annual income range of $0 to $1,000.

Table 2. Incumbents' average first-period choices by treatment

	Unit Cost Low		Unit Cost High	
Treatment	Equalize	Not Equalize	Equalize	Not Equalize
B	64.00%	36.00%	66.67%	33.33%
Y	58.27%	41.73%	57.52%	42.48%

Notes: $N_B = 30$, $N_Y = 28$ and $T = 10$.

Average Behavior

Incumbent and Voter Behavior

To examine the percentage of good (i.e. not corrupt) versus bad (i.e. corrupt) incumbents, we look at the average percentage of incumbents that equalize first-period payoffs within a treatment conditional on the draw of the unit cost of the public good (Table 2). Incumbents in Treatment Y equalize first-period payoffs at similar rates regardless of the level of the unit cost of the public good. These rates tend to be lower than those in Treatment B. On average, 64% of subjects equalize first-period payoffs, which suggest that 36% of incumbents are corrupt.

This percentage, however, only accounts for first-period choices. If we want to estimate the proportion of good incumbents based on the definition proposed in the Besley and Smart (2007) model, we must consider both first- and second-period payoffs, because a good incumbent is defined as one who does not divert rent in either period. Table 3 shows the percentage of incumbents that behave as if bad in the second period conditional on having behaved as if good in the first period. According to the type definitions in the Besley and Smart (2007) model, these percentages should be zero. However, the table shows that this is not the case. In fact, the percentage of incumbents that equalize payoffs in the first period and divert maximum rents in the second period is relatively high (i.e. more than 50% in all cases).

These findings mitigate the empirical validity of the aforementioned definitions and indicate clear strategic behavior on the part of incumbents. We term these choices "theoretically inconsistent" in the context of the Besley and Smart (2007) model, since they signal different types in the first and second

[13] All KS p-values are greater than 0.358. All ES p-values are greater than 0.13.

periods. It should be noted that this behavior is also different from that in the pooling equilibrium derived by Besley and Smart (2007).

Finally, we turn to average voter behavior. Table 4 draws this comparison, where 1.50, 0.75 and 0.16 represent voters' first-period payoffs, as discussed in Table 1.

Table 3. Theoretically inconsistent choices by treatment

Treatment	Unit Cost Low	Unit Cost High
B	56.94%	68.75%
Y	74.58%	71.67%

Notes: $N_B = 30$, $N_Y = 28$ and $T = 10$.

Table 4. Voters' re-election decisions

Treatment	1.50	0.75	0.16
B	61.89	19.52	11.80
Y	76.99	21.25	15.48

Notes: $N_B = 30$, $N_Y = 28$ and $T = 10$.

Treatment Effects

This section focuses on the main treatment effects. First, we specify the econometric model by expressing the estimation equation and defining the variables of interest. Then, we perform estimation and inference. Three comments are noteworthy. First, throughout the discussion, we focus on random-effect (RE) estimators. This choice is mainly dictated by the fact that all of the covariates of interest are individually invariant. Thus, estimation via fixed effects would wipe out the main partial effects (Wooldridge, 2002).

Secondly, a potentially relevant variable (namely history) is not completely controlled for in the regressions below. History of play should not be a major explanatory variable in our regressions, since the experiments were carefully designed to avoid repeated-play effects. Nonetheless, we might expect subjects to play fictitiously.[14] If this is the case, we should control for history accordingly. This would require instrumental-variable (IV) estimation, since the choice history of the incumbents and voters is endogenous. In the absence of proper instruments, we partially control for history by including (1) time and (2) the history of the unit cost (θ_{t-1}) as explanatory variables in the regressions. The time trend is intended to capture any upward or downward trends that may be present in the data. The history of the unit cost acts as an imperfect proxy for history of choices in both the incumbent and voter regressions.[15]

Finally, since we designed the experiments according to a specific structure (i.e. a logical game form), we know a great deal about the data-generating processes. Since the data are generated according to a sequential game of incomplete information by construction (i.e. based on the experimental design), we can rule out with certainty the possibility that decisions are being made simultaneously by the

[14] The modern game-theoretic usage of the term "fictitious play" can be different from the definitions first discussed by Brown (1951) and Robinson (1951). Here we use the term to indicate a situation in which subjects play as if they are paired with the same player every repetition, even though they are told they are not.

[15] The historical value of the unit cost is exogenous, since it is randomly determined by the experimenter's coin flip. Furthermore, the flip directly impacts incumbents' past first-period choices and thus indirectly affects the set of signals that were observable by the voter in the previous period. So, in this sense, the history of the unit cost satisfies both the conditions for an IV or proxy variable (Wooldridge, 2002). However, contrary to the choice history of incumbents and voters, the history of the unit cost is not individually variant. In other words, the history of the unit cost is imperfect as either a proxy variable or an IV. Thus, the question becomes whether to use this variable as a weak instrument or as an imperfect proxy. We use the latter approach, since we expect past choices to have relatively little effect on incumbent and voter behavior due to our re-pairing design.

incumbent and the voter. Thus, we estimate separate equations of interest for each treatment: one for the first mover (the incumbent) and one for the second mover (the voter).

Incumbent Behavior

An individual incumbent's (i) main choice variable in any given repetition (t) is his first-period choice, Choiceit. There are two possible ways to define this variable. The first is to let Choiceit be a dummy variable that is equal to one if the incumbent chooses to equalize the first-period payoffs (i.e. behave as if good). The second alternative is to let Choiceit have a range of three possible values according to equalization, pooling or separation. We herein report findings based on the first formulation, since it lends itself to easier interpretations of the coefficients given the main question.[16]
We estimate the following equation:

$$Choice_{it} = \beta_0 + \beta_1 gen_i + \beta_2 \theta_t + \beta_3 d_Y + \beta_4 bel_{it} + \beta_5 \theta_{t-1} + \beta_6 t + \beta_7 I + \varepsilon_{it} \quad (1)$$

where i = 1, ..., 58, t = 1, ..., 10, β0 represents a constant term, geni represents a dummy variable that is equal to one if the individual is female, θt represents the current draw of the unit cost of the public good, dY represents a dummy variable that is equal to one if the individual is part of the treatment (i.e. yardstick) group, θt−1 is the past lag of the unit cost as a proxy for history, t is a time trend, I is a set of interaction terms consisting of (geni *θt), (geni* dY), (belit * dY), (θt−1 * dY), (geni * θt * dY) and (θt * θt−1), and εit has a one-way error component structure of the form $\varepsilon_{it} = \alpha_i + \nu_{it}$, where αi represents a vector of unobserved individual heterogeneities and νit satisfies the strict exogeneity assumption, $E(\nu_{it}|X_{it}, \alpha_i) = 0$, where Xit is the set of explanatory variables in expression 1.

We also include a proxy variable for beliefs, bel_{it}, on the right-hand side of equation 1. It is expected that when an incumbent makes a first-period choice, he or she anticipates that a particular re-election decision will follow such choice. In other words, an incumbent has some belief towards re-election that is associated with taxes charged. This belief is likely to influence his or her first-period choice, meaning that beliefs can be a statistically significant explanatory variable in the incumbent regressions. Since beliefs are an inherent characteristic of the incumbent, they are unobserved. Furthermore, there is no reason to expect beliefs to be correlated with any of the other explanatory variables in equation 1.

As such, the correct approach to control for beliefs is to use a proxy variable that satisfies the following conditions: (1) irrelevance for explaining the dependent variable, and (2) correlation with beliefs (Wooldridge, 2002). We claim that $Accept_{it}$ (i.e. voters' actual re-election decisions) constitutes a valid proxy for beliefs. First, this variable satisfies requirement (1) since it is chosen by the voter, and thus should not be relevant in explaining the incumbent's choice. Second, the variable satisfies requirement (2) since it is highly correlated with beliefs. This is testable. Under the assumption that the regressors in equation 1 are exogenous and capture all other unexplained variation in $Choice_{it}$, the least-squares residuals from the estimation of equation 1 represent consistent estimates of the error term ε_{it}, and thus of beliefs. A regression of these estimates on $Accept_{it}$ finds that this variable is strongly significant (p-value 0.000).

Table 5 reports the estimates of a linear probability model (LPM).[17] In general, incumbents behave in a more corrupt manner in the presence of yardstick competition; however, female incumbents are more likely than males to equalize first-period payoffs in the presence of yardstick competition. Furthermore, female incumbents are less likely than males to equalize when the unit cost is high. Finally, beliefs are an important determinant of incumbents' choices.

[16] The main treatment effects are robust to alternative formulations, i.e. logit and probit.
[17] The results are robust to alternative estimation methods; i.e. logit and probit.

Table 5. Incumbent regression (Treatment Y)

Dependent Variable: $Choice_{it}$ (1=equalize, 0=not equalize)	
$constant$	0.485 (0.039)
gen_i	0.112 (0.421)
θ_t	0.119 (0.343)
d_Y	-0.313 (0.023)
$beliefs_{it}$	0.372 (0.000)
$history$ (θ_{t-1})	-0.097 (0.441)
t	-0.004 (0.523)
$gen_i * \theta_t$	-0.146 (0.077)
$gen_i * d_Y$	0.330 (0.052)
$beliefs_{it} * d_Y$	0.145 (0.049)
$\theta_{t-1} * d_Y$	-0.008 (0.915)
$gen_i * \theta_t * d_Y$	-0.056 (0.561)
$\theta_t * \theta_{t-1}$	0.037 (0.618)
overall R^2	0.2883
Notes: p-values in parentheses. $N = 58$ and $T = 10$.	

Voter Behavior

An individual voter's (i) main choice variable in any given repetition (t) is his re-election decision, Acceptit, which is a dummy variable that is equal to one if the voter re-elects (accepts) the incumbent. We estimate the following equation:

$$Accept_{it} = \beta_0 + \beta_1 gen_i + \beta_2 pool_{it} + \beta_3 yardstick_{it} + \beta_4 \theta_{t-1} + \beta_5 I + \varepsilon_{it} \quad (2)$$

where i =1, ..., 28, t =1, ..., 10, β0 represents a constant term, geni represents a dummy variable that is equal to one if the individual is female, poolit represents a dummy variable that is equal to one if the voter observes a pooling first-period payoff, θt−1 is the past lag of the unit cost as a proxy for history, I is an interaction term (geni ∗ poolit), and εit has a one-way error component structure of the form $\varepsilon_{it} = \alpha_i + \nu_{it}$, where αi represents a vector of unobserved individual heterogeneities and εit satisfies the strict exogeneity assumption, $E(\nu_{it}|X_{it}, \alpha_i) = 0$, where Xit is the set of explanatory variables in expression 2.

There are some subtleties in equation 2. First, since the yardstick sessions are identical to the baseline sessions except for one minor modification, the immediate question arises how to model this subtle change if we want to compare the baseline and yardstick data. One alternative is to include a dummy that equals one if the voter subject is in the treatment group. This formulation leads to the conclusion that yardstick competition has no effect.

Another alternative is to define a variable that takes into account the nature of the yardstick information. In this case we are unable to collapse the data from Treatment B and Treatment Y, since any yardstick variable that incorporates the nature of the yardstick signal is undefined in Treatment B. In other words, if we want to appeal to the yardstick nature of the Y sessions by defining a yardstick variable, the above regression will be based only on data from the Y sessions (i.e. N = 28). So, the question we ask in this formulation is whether the yardstick variable captures any treatment effects given the assumption that Treatments B and Y are otherwise identical. This assumption is reasonable, since the baseline and yardstick treatments were designed to be identical except for the presence of yardstick information. Therefore, if equation 2 controls for the same set of covariates as a baseline regression, the yardstick variable should capture consistent treatment effects.

The second question is how to define the yardstick variable. The findings are robust to two alternative definitions. In particular, treatment effects arise whether we define the yardstick variable as (1) the difference between the tax rate in one's own jurisdiction and the tax rate in the other jurisdiction or (2) a dummy that equals one if the tax rates are the same. We report results for the former. We estimate equation 2 for three separate cases (when the signal is 1.50, 0.75 and 0.16). Each estimation is based on a subset of repetitions, since the yardstick signal can only take on a particular value in the first period of any given repetition.

Table 6 reports the estimates of a linear probability model (LPM) for the three cases.[18] The main result arising from this table is that there is strong evidence of yardstick competition. In particular, voters pay attention to the difference between the tax rate in another jurisdiction and their own. Furthermore, any deviation from the signal indicating that they are worse off in comparison to the other jurisdiction is punished by voting out (rejecting) the incumbent.

To interpret the yardstick competition coefficient, recall that the yardstick variable is defined as the difference between the yardstick signal and the voter's first-period payoff. Therefore, if the signal equals 1.50, then the yardstick variable has a nonnegative range. In particular, the variable can take on the values 0.00 (1.50 minus 1.50), 0.75 (1.50 minus 0.75) or 1.34 (1.50 minus 0.16), and an increase in the yardstick variable from zero to positive automatically tells us that the first-period payoff is below the yardstick signal. Thus, a negative sign on the $yardstick_{it}$ coefficient tells us that the voter is more likely to vote out the incumbent when first-period welfare is below that seen in the other jurisdiction. A similar reasoning holds when the signals are 0.75 and 0.16.

Table 6. Voter regression (Treatment Y)

Dependent Variable: $Accept_{it}$ (1=re-elect, 0=vote out)			
	1.50 ($T = 2$)	0.75 ($T = 4$)	0.16 ($T = 4$)
$constant$	0.871 (0.001)	0.069 (0.650)	-0.118 (0.478)
gen_i	-0.048 (0.769)	0.022 (0.859)	-0.064 (0.659)
$pool_{it}$	0.084 (0.699)	0.143 (0.332)	0.128 (0.234)
$yardstick_{it}$	-0.522 (0.000)	-0.235 (0.034)	-0.476 (0.000)
$history$ (θ_{t-1})	-0.033 (0.801)	0.283 (0.001)	0.180 (0.019)
$gen_i * pool_{it}$	0.165 (0.546)	0.203 (0.289)	0.057 (0.706)
$gen_i * yardstick_{it}$	-0.051 (0.774)	-0.309 (0.037)	-0.076 (0.669)
overall R^2	0.4591	0.3827	0.3219
Notes: p-values in parentheses. $N = 28$.			

[18] These findings are robust to alternative estimation methods; i.e. logit and probit.

Summary and Related Literature

This section summarizes the main findings (i.e. treatment and partial effects) and relates them to the previous literature. We first discuss findings on the incumbent side. Then, we proceed to findings on the voter side. Finally, we relate the gender effects to previous findings and discuss the generalizability of the results.

Incumbents

Two main treatment effects are identified in the present study. The first is that female incumbents behave less corruptly in the presence of yardstick competition compared to males. In other words, yardstick competition acts as a corruption-taming mechanism if the incumbent is female. The second is that incumbents exposed to Treatment Y care more about beliefs than incumbents exposed to the baseline conditions. This indicates that the mere presence of a benchmark makes incumbents more concerned about their choice of tax rates. In particular, the sign of the coefficient (which is positive) suggests that incumbents are more likely to choose a particular tax rate in the presence of yardstick competition if they believe it to be associated with a higher likelihood of re-election. Thus, even though yardstick information is non-existent on the incumbent's side, it affects the incumbent's perspective towards re-election, and therefore affects his or her choices. This gives support to the claim of Besley and Case (1995) that incumbent behavior (i.e. vote seeking) and tax setting are tied together through yardstick competition.

In addition to these main treatment effects, we also find the following partial effects. First, the unit cost of the public good pushes incumbents towards equalization when it takes on a high value. This is likely due to the fact that incumbents have the ability to fool the voter by pooling when the unit cost is low. When the unit cost is high, however, the incumbent must either equalize or separate. If the incumbent chooses the latter by diverting maximum rents and charging maximum taxes, the voter is likely to punish by voting out the incumbent. Therefore, an incumbent who behaves strategically and is forward-looking would rather divert zero rents and equalize payoffs when the unit cost is high, in an attempt to increase his or her chances of being re-elected.

Secondly, gender in combination with the unit cost is also significant. When the unit cost is high, female incumbents are less likely to equalize. This could occur because female incumbents anticipate being voted out anyway when the unit cost is high. Namely, if the incumbent diverts zero rent when the unit cost is high, there is a still a chance that the voter will not re-elect, since the voter may think that the incumbent is trying to fool him or her by pooling. Therefore, female incumbents may separate at a higher rate in anticipation of rejection. Finally, beliefs are important. Incumbents choose tax rates that they believe are associated with a higher likelihood of re-election.

Voters

There is one main treatment effect: Voters are less likely to re-elect in the presence of yardstick competition conditional on the yardstick signal. In particular, voters exposed to Treatment Y fixate on the difference between the tax rate in their jurisdiction versus that in the other jurisdiction. As this difference increases (i.e. as the voter's own tax rate is deemed more unfair compared to the benchmark), voters are more likely to vote out the incumbent. This suggests that voters process yardstick information in a specific manner by focusing on the difference between tax rates. This finding supports Besley and Case (1995) in their claim that voters focus on the difference between the tax rate in their own jurisdiction and that in another.

The data also indicate that when the yardstick signal is 0.75, female voters are more likely to oust the incumbent in the presence of yardstick competition. Finally, there are two additional partial effects. First, the main determinant of whether or not the voter re-elects the incumbent is the tax rate. A lower tax rate (i.e. higher payoff) is strongly (and logically) associated with a higher re-election rate. Secondly, history (i.e. past draw of the unit cost) affects re-election decisions. The voter is more likely to re-elect the incumbent this period as the past draw of the unit cost increases.

Gender

In systematically exploring previous and new findings on gender, Cox and Deck (2006) conclude that women are more responsive to the total economic and social costs of generous behavior, compared to men. In particular, the authors find that women tend to be more generous (first-mover behavior) and more reciprocal (second-mover behavior) in low social distance and high payoff scenarios. In order to relate our findings to theirs, it is necessary to extend their terminology to our context.[19] Comparing Treatments B and Y, we can argue that the yardstick treatments impose a greater social cost on the incumbent than the baseline treatments. After all, the main aim of introducing yardstick information is to give voters a benchmark to which they can compare their respective incumbents. Therefore, it is not surprising that female incumbents are more likely to equalize (i.e. behave non-corruptly) in the presence of yardstick competition, because the costs of ungenerous behavior are higher in this case.

Alternatively, we can also make a distinction between high and low payoff states. In particular, when the unit cost is high, first-period payoffs are lower than when the unit cost is high. This would argue that females would have to be more generous (i.e. less corrupt) when the unit cost is high. However, this hypothesis is not supported by our data, potentially because both economic and social cost components would be necessary to observe gender behavior in a particular direction. On the voter side, it is less clear how the yardstick information affects economic and social costs. Depending on the voter's tax rate, the yardstick signal may or may not increase the social costs. Therefore, we might not expect to see a gender effect. In fact, the data suggest that there is a gender effect only when the signal is 0.75. Overall, these findings indicate that the differences are not just a matter of gender, but also the decision-making context.

Generalizability

The fact that the findings of our laboratory experiments line up with the findings of Besley and Case (1995) indicates parallelism with the external naturally occurring environment. In particular, we reach the same conclusion regarding the effect of yardstick competition on behavior, regardless of whether we base our conclusion on the field or laboratory data. This illustrates that even though pro-social behavior may be biased in a laboratory setting, these types of laboratory experiments can still capture consistent and generalizable treatment effects, particularly if the design is informed by a model. In fact, Levitt and List (2007) in part allude to this, by commenting on the adequacy of laboratory experiments to test general theories.[20]

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[19] We can hypothesize on how to interpret the gender effects in the context of the Besley and Smart (2007) model. However, to develop a formal model of gender differences, we must find support for whether or not these differences are determined by nature or nurture. For example, if more females are by nature altruistic, we could argue that nature determines the proportion of good female incumbents to be higher than the proportion of good male incumbents. Alternatively, if females have more altruistic preferences (possibly determined by nurture or social norms), this would argue for the presence of gender-dependent preference specifications. While these are interesting questions, they are beyond the scope of this study. Thus, the gender dummies should be interpreted in the context of previous findings as opposed to the theoretical model.

[20] Armantier and Boly (2007) directly address the question of corruption in the laboratory and field. They also find comparable treatment effects.

4. CONCLUSION

This study reports theory-testing laboratory experiments on the effect of yardstick competition on corruption. On the incumbent's side, yardstick competition acts as a corruption-taming mechanism if the incumbent politician is female. In addition, yardstick competition makes incumbents more aware of their choices, since beliefs matter more in the presence of yardstick competition.

On the voter's side, voters focus on the difference between the tax rate in their own jurisdiction versus that in another jurisdiction when making re-election decisions. If the tax rate in their own jurisdiction is deemed unfair compared to the one in another jurisdiction, voters are less likely to re-elect. This suggests that voters use yardstick information in a specific manner, and that the overall difference between tax rates matters.

The experiments also find some partial effects that determine corruptibility. On the incumbent's side, the unit cost of the public good matters. If the unit cost is high, incumbents are more likely to charge low taxes. Also, gender is important when interacted with the unit cost. Female incumbents are more likely to divert rents and charge maximum taxes when the unit cost is high. Finally, incumbents' beliefs towards re-elections are important. Incumbents weigh the likelihood of re-election heavily when choosing tax rates. On the voter's side, beliefs are captured by the tax rate, which is a signal about the incumbent's nature. In particular, voters re-elect more frequently when taxes are lower.

Overall, these findings support the seminal claim by Besley and Case (1995) that incumbent behavior (i.e. vote seeking) and tax setting are tied together through the nexus of yardstick competition. This renders generalizability to the findings of our laboratory experiments.

APPENDIX: YARDSTICK COMPETITION: SUBJECT INSTRUCTIONS

Remark 1: *These instructions have been slightly adapted. The following changes have been made: (1) the figures and text have been reduced in size and (2) the preliminaries section has been included for the baseline treatment only.*

INSTRUCTIONS

Preliminaries

- Hello and welcome.
- You are now taking part in an economics experiment.
- Please read these instructions carefully.
- We ask that .as of this point .you no longer talk to each other.
- Please turn off all cellular phones, two-way pagers and any other electronic devices.
- This set of instructions will explain the steps that are involved in taking part in this economics experiment. Everything you need to know to participate in this experiment is explained below.
- If you have any questions about these instructions or about any issue during the experiment, please raise your hand. We will come to you and answer your questions at your cubicle.

General

- This experiment consists of four sections: (1) a trial, (2) a quiz, (3) a task called Task B and (4) a questionnaire.
- Task B requires interaction between two players: Player X and Player Z.
- Before you make any decisions, you will be told whether you are Player X or Player Z. You will keep the same role for the entire experiment.
- Please pay very close attention to all of the instructions.

Task B

1. There are ten periods in this task. Each period has two parts, a first and a second part.
2. Each period you will be paired with a DIFFERENT person in the room. So, you will NOT interact with the same person for more than one period.
3. At the beginning of the .first part of the period, a coin is flipped. Player X knows the result of the coin flip. Player Z does not. Player X should NOT reveal the result of the coin flip to Player Z. The coin flip takes place in front of the first Player X. He verifies the coin flip and I input it into the computer.
4. If the coin flip is "HEADS," Player X chooses between THREE alternatives; if it is "TAILS," TWO alternatives.
5. Each alternative is a payoff pair: The first is Player X's; the second is Player Z's.
6. Suppose the coin lands "HEADS." Player X sees the screen in figure 1. Please look at the screen and the payoffs. The first column is the respective alternative. The second and third

columns are Player X's and Player Z's payoffs. If Player X chooses alternative 1, Player X and Player Z get 1.50. If Player X chooses alternative 2, Player X gets 1.50 and Player Z gets 0.75. Finally, if Player X chooses alternative 3, Player X gets 2.35 and Player Z gets 0.16.

Figure 1. Player X sample screen: First part of period (Three alternatives)

7. Suppose the coin lands "TAILS." Player X sees the screen in figure 2. The screen is similar to the previous; but, Player X chooses between TWO rather than THREE alternatives. Also, the payoffs are different.

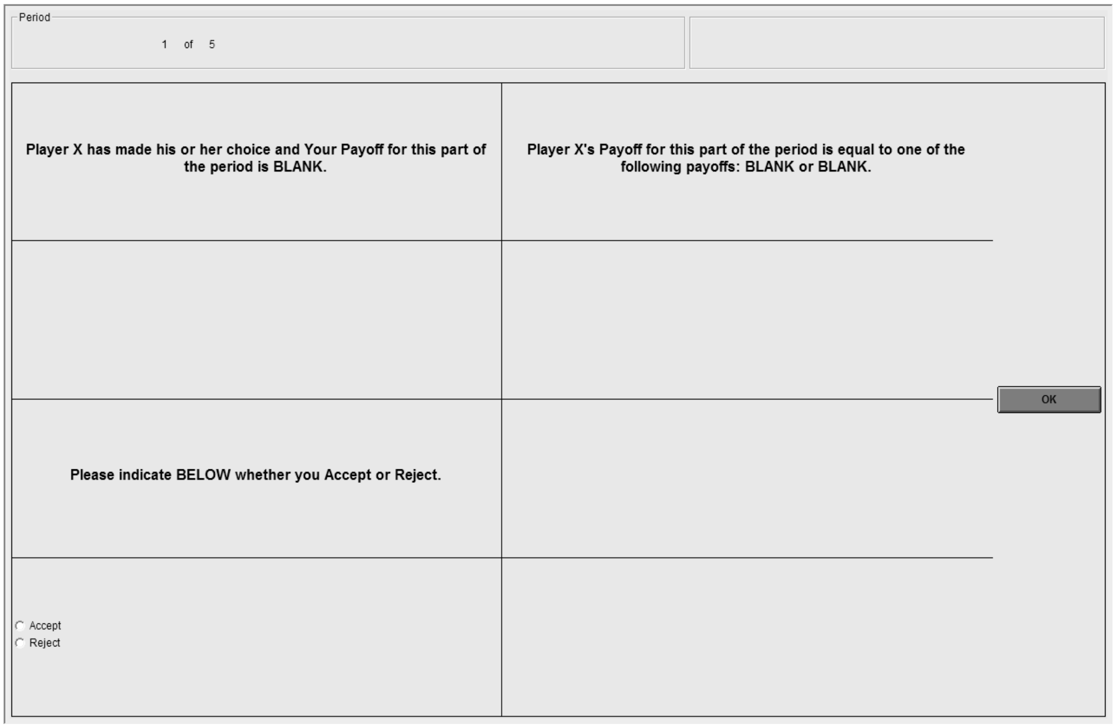
Figure 2. Player X sample screen: First part of period (Two alternatives)

8. Player X selects his choice and clicks OK to confirm. This choice determines payoffs for the first part of the period.
9. Player Z does NOT see the coin flip. So, Player Z does NOT see whether Player X chose between the THREE alternatives in figure 1 or the TWO alternatives in figure 2.
10. Player Z sees a screen like the one in figure 3. Figure 3 is a sample screen. Please look at the screen in figure 3.
11. The first cell in the first row indicates Player Z’s payoff¤. In the sample screen, the payoff is indicated as "BLANK." This number can be either1.50, 0.75 or 0.16 depending on the alternative chosen by Player X.
12. The second cell in the first row indicates Player X’s possible payoffs. In the sample screen, the possible payoffs are indicated as "BLANK." In the actual task, Player X’s possible payoffs depend on Player Z’s payoff as shown in the following table.

	If Player Z has	**Then, Player X has**
Case a	1.50	1.50
Case b	0.75	1.50 OR 0.75
Case c	0.16	2.35 OR 1.35

So, if Player Z has a first-part payoff of 1.50, then Player X has a .first-part payoff of 1.50 also. If Player Z has a first-part payoff of 0.75, then Player X could also have a first-part payoff of 0.75, but Player X could have a first-part payoff of 1.50 instead. This depends on the number of alternatives that Player X chose from. Finally, if Player Z has a first-part payoff of 0.16, then Player X either has a first-part payoff of 2.35 or 1.35. This depends on the number of alternatives that Player X chose from.

Figure 3. Player Z sample screen: Accept/reject screen



13. Player Z "accepts" or "rejects" at the end of the first part of the period. Player Z selects his choice at the bottom of the screen and clicks OK to confirm. The decision to "accept" or "reject" determines whether or not Player X makes another choice in the second part of the period. It does NOT determine or affect first-part payoff for either player. First-part payoffs are whatever Player X chose them to be.
14. So, if Player Z "rejects," both players still get the payoffs chosen by Player X in the first part of the period. However, Player X does NOT make a choice in the second part of the period. Instead, Player X's payoff for the second part of the period is zero and Player Z's second-part payoff is determined randomly as indicated in the following table.

If Player Z "rejects," Player Z's Second-Part Payoff¤ is:
1.50 with 25% chance
0.75 with 25% chance
0.16 with 50% chance

15. On the other hand, if Player Z "accepts," both players still get the payoffs chosen by Player X in the first part of the period. But, in addition, Player X makes a choice in the second part of the period and this choice determines second-part payoffs for both players. The computer will pause and I will flip the coin again. This second coin toss affects the alternatives that Player X chooses from in the second part of the period. This second coin flip will be verified in the same way as the first coin flip. Players Z will NOT see the result of this coin flip.
16. If the coin lands "HEADS," Player X sees the screen in figure 4. Please look at the screen and the payoffs.

Figure 4. Player X sample screen: Second part of period

17. If the coin lands "TAILS," Player X sees the screen in Figure 5. Please look at the screen and the payoffs.
18. Player X's second-part choice determines Player X's and Player Z's payoffs for the second part. Player Z will NOT get to "accept" or "reject" at the end of the second part of the period.
19. The above steps describe the two parts within a period. This task consists of ten of these periods.

Figure 5. Player X sample screen: Second part of period

Task Payoffs

Payoffs in each period are the sum of the payoffs in the first and the second part of the period. At the end of each period, you will see how much you earned during that period and any previous periods. You will be paid your total earnings for all periods. All payoffs shown are in experimental dollars. Each experimental dollar is a quarter of a real US dollar. So, for example, if you make 8 experimental dollars in this task, you will get paid 2 real US dollars for this task at the end of the experiment.

Waiting Involved

Note that each decision round is complete when everyone participating in the experiment (i.e. all Players X and all Players Z) has submitted a decision. This means that the delay of a single participant's decision can prevent the next round from starting. Please take time to make your decision, but please also keep the above in mind.

Questions

Are there any questions before we discuss the Trial?

Trial & Quiz

Now, we will have five minutes of what is called a trial. During this trial, you will NOT be assigned a role. This means that you will get to make choices as if you were Player X. Then, you will get to observe those choices as if you were Player Z. During this process, you will get to see the screens that both Players X and Z will get to see once Task B starts. During this trial, you will NOT be interacting with any other player. So, the trial is a practice opportunity for you to familiarize yourself with the software and the instructions.

The trial will last five minutes. If you have any questions during the trial period, please raise your hand. We will come to your cubicle and answer your questions.

After the trial is completed, you will be given a quiz on the computer to make sure you understand the procedures and how your earnings are determined. The questions in the quiz are based on the trial and the instructions.

You will be paid 3.00 REAL US dollars for participating in this trial and completing the quiz REGARDLESS of how many trial periods you complete or how many quiz questions you answer correctly. So, please take your time and pay attention to the screens you see in the trial and the questions you are asked in the quiz.

Are there any questions before we start the Trial?

Task B Summary

1. You will randomly be Player X or Player Z. You will keep the same role for the entire experiment.
2. At the beginning of the first part of the period, a coin is flipped.
3. Player X knows the result of the coin flip and knows his alternatives. If the coin toss is "HEADS" there are THREE alternatives; if the coin toss is "TAILS" there are TWO alternatives.
4. Player X makes his first-part choice. For each possible choice, Player X knows the first-part payoffs for himself AND for Player Z.
5. Player Z does NOT know the result of the coin toss and does not know the number of alternatives faced by Player X.
6. Player Z knows his first-part payoff for sure, but does not know Player X's first-part payoff. The following table, which is also displayed on the screen at the front of the room, shows the possible first-part payoffs for Player X depending on Player Z's first-part payoff.

	If Player Z has….	**Then, Player X has…**
Case a	1.50	1.50
Case b	0.75	1.50 OR 0.75
Case c	0.16	2.35 OR 1.35

7. Player Z "accepts" or "rejects." This decision determines whether or not Player X makes another choice in the second part of the period. It does NOT determine or affect first-part payoffs for either player. First-part payoffs are whatever Player X chose them to be.
8. If he "accepts," both players get the first-part payoffs chosen by Player X, but in addition. Player X sees another coin toss and makes a choice that determines payoffs in the second part. Player Z does NOT "accept" or "reject" for a second time.

9. If he "rejects," both players get the first-part payoffs chosen by Player X, but Player X’s second-part payoff is zero. Player Z’s second-part payoff is determined randomly. The possibilities are shown in the table below.
10. The above describes the two parts within a period. This task consists of ten of these periods.

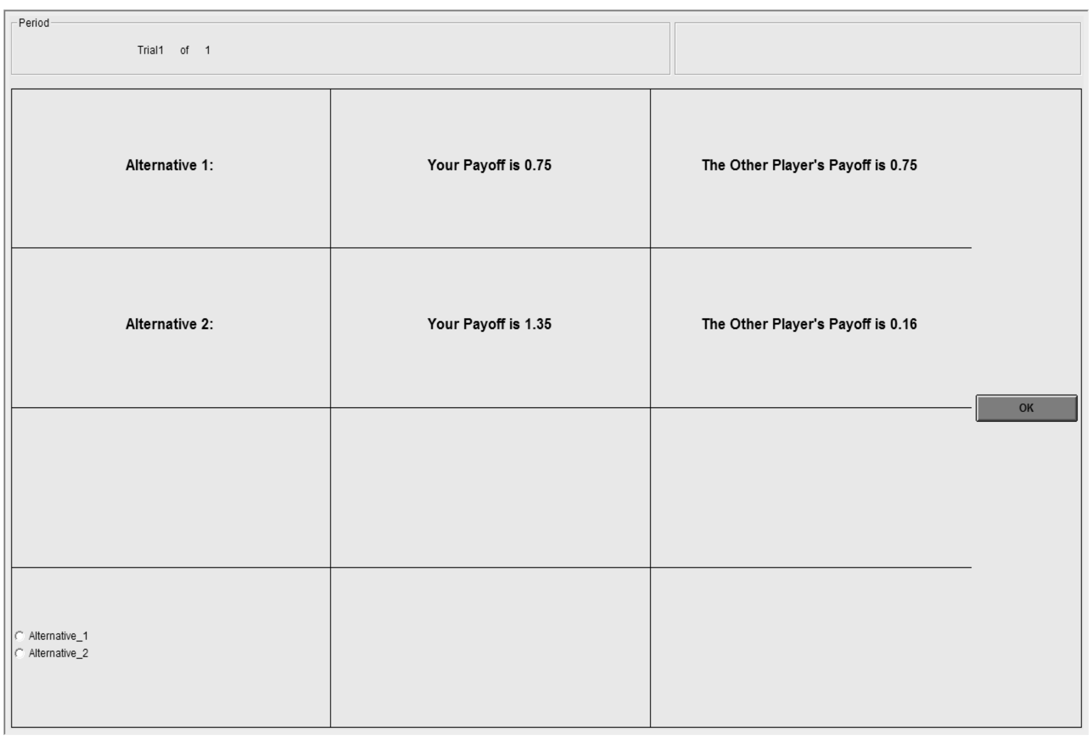
If Player Z "rejects," Player Z’s Second-Part Payoff is:
1.50 with 25% chance
0.75 with 25% chance
0.16 with 50% chance

Task Y

1. There are ten periods in this task. Each period has two parts, a first and a second part.
2. Each period you will be paired with a DIFFERENT person in the room. So, you will NOT interact with the same person for more than one period.
3. At the beginning of the .first part of the period, a coin is flipped. Player X knows the result of the coin flip Player Z does not. Player X should NOT reveal the result of the coin flip to Player Z. The coin flip takes place in front of the first Player X. He verifies the coin flip and I input it into the computer.
4. If the coin .flip is "HEADS," Player X chooses between THREE alternatives; if it is "TAILS," TWO alternatives.
5. Each alternative is a payoff pair: The first is Player X’s; the second is Player Z’s.
6. Suppose the coin lands "HEADS." Player X sees the screen in Figure 6. Please look at the screen and the payoffs. The first column is the respective alternative. The second and third columns are Player X’s and Player Z’s payoffs. If Player X chooses alternative 1, Player X and Player Z get 1.50. If Player X chooses alternative 2, Player X gets 1.50 and Player Z gets 0.75. Finally, if Player X chooses alternative 3, Player X gets 2.35 and Player Z gets 0.16.
7. Suppose the coin lands "TAILS." Player X sees the screen in figure 7. The screen is similar to the previous; but, Player X chooses between TWO rather than THREE alternatives. Also, the payoffs are different.
8. Player X selects his choice and clicks OK to confirm. This choice determines payoffs for the first part of the period.
9. Player Z does NOT see the coin flip. So, Player Z does NOT see whether Player X chose between the THREE alternatives in figure 6 or the TWO alternatives in figure 7.
10. Player Z sees a screen like the one in figure 8. Figure 8 is a sample screen. Please look at the screen in figure 8.

Figure 6. Player X sample screen: First part of period (Three alternatives)

Figure 7. Player X sample screen: First part of period (TWO alternatives)

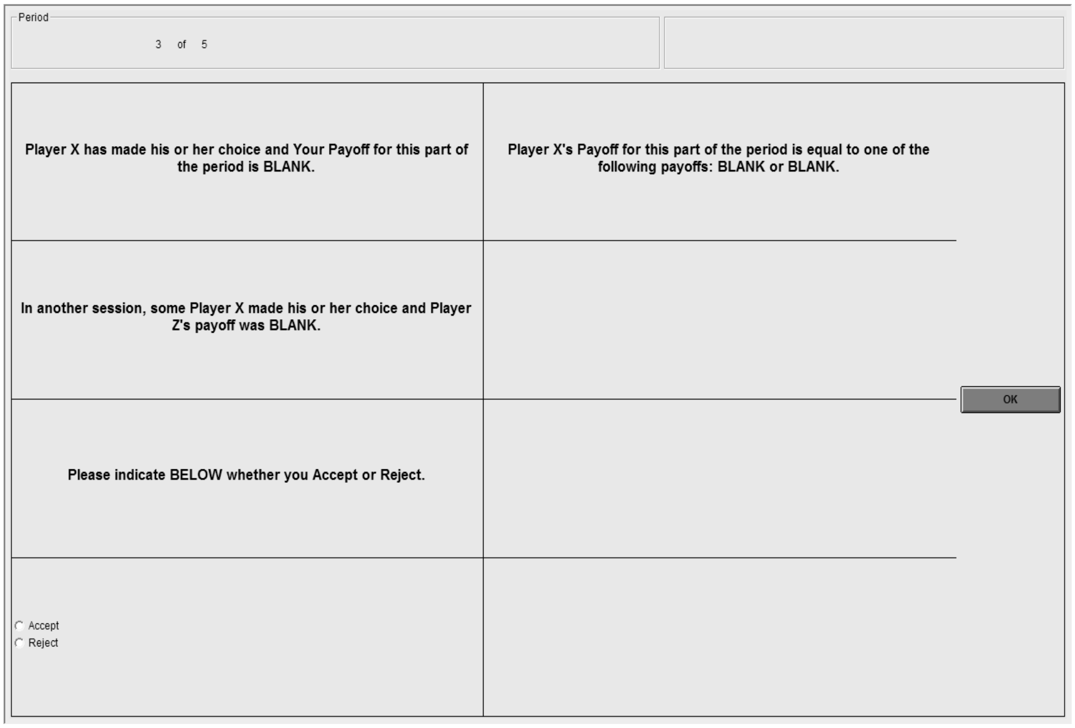


- The first cell in the first row indicates Player Z's payoff. In the sample screen, the payoff is indicated as "BLANK." This number can be either 1.50, 0.75 or 0.16 depending on the alternative chosen by Player X.
- The second cell in the first row indicates Player X's possible payoffs. In the sample screen, the possible payoffs are indicated as "BLANK." In the actual task, Player X's possible payoffs depend on Player Z's payoffs as shown in the following table.

	If Player Z has...	Then, Player X has...
Case a	1.50	1.50
Case b	0.75	1.50 OR 0.75
Case c	0.16	2.35 OR 1.35

- So, if Player Z has a first-part payoff of 1.50, then Player X has a first-part payoff of 1.50 also. If Player Z has a first-part payoff of 0.75, then Player X could also have a first-part payoff of 0.75, but Player X could have a first-part payoff of 1.50 instead. This depends on the number of alternatives that Player X chose from. Finally, if Player Z has a first-part payoff of 0.16, then Player X either has a first-part payoff of 2.35 or 1.35. This depends on the number of alternatives that Player X chose from.
- The first cell in the second row shows information from a session similar to the one that you are in today. This session was called session B. The cell shows a particular Player Z's first-part payoff in session B. This was the result of a choice made by the Player X that he was paired with. Session B was the same as today's session in terms of procedures: A coin was flipped and verified by the first Player X and Player Z did NOT get to see the coin flip. Furthermore, if the coin landed "HEADS," Player X chose from the screen in figure 6. If the coin landed "TAILS," Player X chose from the screen in figure 7. However, session B was potentially different from today's session in terms of the result of the coin flip. In other words, while you see another Player Z's first-part payoff coming from session B, you do NOT know whether the Player X that made this choice saw the same coin flip as the Player X you are paired with. So, your Player X could have observed "HEADS" while the other Player X observed "TAILS" or vice versa. In the sample screen below, the other Player Z's first-part payoff is indicated as "BLANK." During the task, this number is either 1.50, 0.75 or 0.16. The first-part payoff shown in the cell has been chosen at random from all Player Z first-part payoffs in session B in any given period.

Figure 8. Player Z sample screen: Accept/reject screen



11. Player Z "accepts" or "rejects" at the end of the first part of the period. Player Z selects his choice at the bottom of the screen and clicks OK to confirm. The decision to "accept" or "reject" determines whether or not Player X makes another choice in the second part of the period. It does NOT determine or affect first-part payoffs for either player. First-part payoffs are whatever Player X chose them to be.
12. So, if Player Z "rejects," both players still get the payoffs chosen by Player X in the first part of the period. However, Player X does NOT make a choice in the second part of the period. Instead, Player X’s payoff for the second part of the period is zero and Player Z’s second-part payoff is determined randomly as indicated in the following table.

If Player Z "rejects," Player Z’s Second-Part Payoff¤ is:
1.50 with 25% chance
0.75 with 25% chance
0.16 with 50% chance

13. On the other hand, if Player Z "accepts," both players still get the payoffs chosen by Player X in the first part of the period. But, in addition, Player X makes a choice in the second part of the period and this choice determines second-part payoffs for both players. The computer will pause and I will flip the coin again. This second coin toss affects the alternatives that Player X chooses from in the second part of the period. This second coin flip will be verified in the same way as the first coin flip. Players Z will NOT see the result of this coin flip.
14. If the coin lands "HEADS," Player X sees the screen in figure 9. Please look at the screen and the payoffs

Figure 9. Player X sample screen: Second part of period

15. If the coin lands "TAILS," Player X sees the screen in figure 10. Please look at the screen and the payoffs

Figure 10. Player X sample Screen: Second part of period

16. Player X's second-part choice determines Player X's and Player Z's payoffs for the second part. Player Z will NOT get to "accept" or "reject" at the end of the second part of the period.
17. The above steps describe the two parts within a period. This task consists of ten of these periods.

Task Payoffs

Payoffs in each period are the sum of the payoffs in the first and the second part of the period. At the end of each period, you will see how much you earned during that period and any previous periods. You will be paid your total earnings for all periods. All payoffs shown are in experimental dollars. Each experimental dollar is a quarter of a real US dollar. So, for example, if you make 8 experimental dollars in this task, you will get paid 2 real US dollars for this task at the end of the experiment.

Waiting Involved

Note that each decision round is complete when everyone participating in the experiment (i.e. all Players X and all Players Z) has submitted a decision. This means that the delay of a single participant's decision can prevent the next round from starting. Please take time to make your decision, but please also keep the above in mind.

Questions

Are there any questions before we discuss the Trial?

Trial & Quiz

Now, we will have five minutes of what is called a trial. During this trial, you will NOT be assigned a role. This means that you will get to make choices as if you were Player X. Then, you will get to observe those choices as if you were Player Z. During this process, you will get to see the screens that both Players X and Z will get to see once Task Y starts. During this trial, you will NOT be interacting with any other player. So, the trial is a practice opportunity for you to familiarize yourself with the software and the instructions.

The trial will last five minutes. If you have any questions during the trial period, please raise your hand. We will come to your cubicle and answer your questions.

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You will be paid 3.00 REAL US dollars for participating in this trial and completing the quiz REGARDLESS of how many trial periods you complete or how many quiz questions you answer correctly. So, please take your time and pay attention to the screens you see in the trial and the questions you are asked in the quiz.

Are there any questions before we start the Trial?

Task Y Summary

1. You will randomly be Player X or Player Z. You will keep the same role for the entire experiment.
2. At the beginning of the first part of the period, a coin is flipped.
3. Player X knows the result of the coin flip and knows his alternatives. If the coin toss is "HEADS" there are THREE alternatives; if the coin toss is "TAILS" there are TWO alternatives.
4. Player X makes his first-part choice. For each possible choice, Player X knows the first-part payoffs for himself AND for Player Z.

5. Player Z does NOT know the result of the coin toss and does not know the number of alternatives faced by Player X.
6. Player Z knows his first-part payoff for sure, but does not know Player X's first-part payoff. The following table, which is also displayed on the screen at the front of the room, shows the possible first-part payoffs for Player X depending on Player Z's first-part payoff.

	If Player Z has...	Then, Player X has...
Case a	1.50	1.50
Case b	0.75	1.50 OR 0.75
Case c	0.16	2.35 OR 1.35

7. Player Z also knows the first-part payoff of some Player Z in another session. The procedures in this session were the same as in the experiment today. While Player Z sees this other Player Z's first-part payoff, he does NOT know whether the Player X that chose it saw the same coin flip as the Player X that he is paired with.
8. Player Z "accepts" or "rejects." This decision determines whether or not Player X makes another choice in the second part of the period. It does NOT determine or affect first-part payoffs for either player. First-part payoffs are whatever Player X chose them to be.
9. If he "accepts," both players get the first-part payoffs chosen by Player X, but in addition Player X sees another coin toss and makes a choice that determines payoffs in the second part. Player Z does NOT "accept" or "reject" for a second time.
10. If he "rejects," both players get the first-part payoffs chosen by Player X, but Player X's second-part payoff is zero. Player Z's second-part payoff is determined randomly. The possibilities are shown in the table below.

If Player Z "rejects," Player Z's Second-Part Payoff is:
1.50 with 25% chance
0.75 with 25% chance
0.16 with 50% chance

11. The above describes the two parts within a period. This task consists of ten of these periods.

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